**Statement on the HST/JWST Transition Plan**

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July 31, 2003

Ever since it was decided to move on from Woodhenge to Stonehenge (if that is indeed the relation of these monuments), decisions on when to stop supporting a major astronomical facility in favor of newer ones have been difficult. Nor is the problem unique to astronomy. It is useful, therefore, to seek insights from a broad perspective.

Thomas Kuhn (1962) published a highly influential description of the process by which science advances (*The Structure of Scientific Revolutions* - despite its highly academic topic, a million copies have been sold). He showed how an immature field of science has little unity, until it advances sufficiently to adopt a generally accepted paradigm. Thereafter, efforts are channeled into exploring within the limits of the paradigm, a process he calls “normal science.” As he describes it: “One of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. ..One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving.” (p. 37)

Paradigms as defined by Kuhn apply to subfields of a science as well – and in many cases they become established firmly as advances. Adapting the views of Karl Popper (another major contributor to philosophy of science), the long intervals of Kuhn’s normal science work to corroborate aspects of a paradigm through failed efforts to prove them false. It must be "not to save the lives of untenable systems, but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival” (Popper 1934, p. 42). Fundamental advances await a dramatic failure, resulting eventually in a new paradigm. Thereafter, the process repeats.

Within astronomy, we celebrate paradigm shifts like the Copernican/Keplerian Revolution, the identification of external galaxies, the discovery of active galactic nuclei, observing the cosmic redshift, establishing the nature of quasi-stellar objects, the merger of particle physics and early cosmology, the discovery of infrared galaxies, and showing there are massive planets around many stars. Kuhn (2000, p. 141) points out that “it is technical puzzles that provide the usual occasion and often the concrete materials for revolution. Their availability together with the information and signals they provide account in large part for the special nature of scientific progress.” Astronomy provides many illustrative examples.

In fact, in “Cosmic Discovery” (1981), Martin Harwit assesses the impact of new technology on major discoveries in astronomy, advances equivalent to Kuhn’s sub-field paradigm shifts. For recent discoveries, he finds that the enabling technology was less than ten years old in virtually every case. He concludes:

- The most important observational discoveries result from substantial technological innovation…

- Once a powerful new technique is applied in astronomy, the most profound discoveries follow with little delay…
- A novel instrument soon exhausts its capacity for discovery…

The questions faced by the ***HST-JWST Transition Plan Review Panel*** need to be considered in the context of space astronomy as a whole. Starting from the first HST servicing mission, without servicing it is likely that the telescope will be used for at least 16 years of full capability research. Given its high productivity, extending this period by another 4 – 5 years is obviously desirable, if considered alone. However, it is likely that the costs of doing so will have negative impacts not only on JWST, but also on future missions such as SAFIR and SUVO.

These impacts need to be measured in terms of the opportunities for generating paradigm shifts, not just on the number of "normal science" papers generated per year. A major effort in new instrumentation would be required for HST to have much potential for paradigm shifting. Given the quality of the instruments currently in use, and the advanced state of optical and near-ultraviolet detectors, even new instruments will generally only bring moderate improvements in speed, not qualitatively new capabilities.

To compare with the potential for JWST, I use the "astronomical capability" metric from the Bahcall Decadal Survey. This metric can be described as the relative mapping time for an area measured in pixels on the sky and to a given sensitivity limit for point sources. An extended HST mission with existing instruments would provide increases in this metric by factors of order 1.5 to 2. In contrast, and as shown in Figure 1, the gain provided by JWST is many orders of magnitude, implying a high potential for new discoveries and paradigm shifts.

Of course, it is unlikely that the funds needed for a HST refurbishment will deny us JWST. However, a consequence of diverting funds to provide extended operations for HST will be to stretch out the JWST development compared with the optimum schedule. JWST is likely to encounter significant increased costs as a result, and of course the realization of its potential will be delayed.

The real impact of an extended HST mission will probably fall on other possible future NASA missions like SUVO, SAFIR, or even TPF. Particularly allowing for the additional needs for a slowed-down JWST, the money spent for an extended HST mission could advance one of these other missions substantially toward reality. The menu of future missions already has more entries than NASA may be able to afford. In addition, we tend to forget in good times for Space Science that bad times can occur – as in the early 1980s, when huge overruns by HST and other missions and a general disorganization of Space Science led to virtually no major New Starts for nearly a decade – or as in the early to mid 1990s, when the on-orbit problems of HST, Galileo, and Mars Observer combined with a national recession to bring the entire future of Space Science into question. It is possible that the ultimate consequence of extending the life of HST would be that SUVO or SAFIR would never be built.

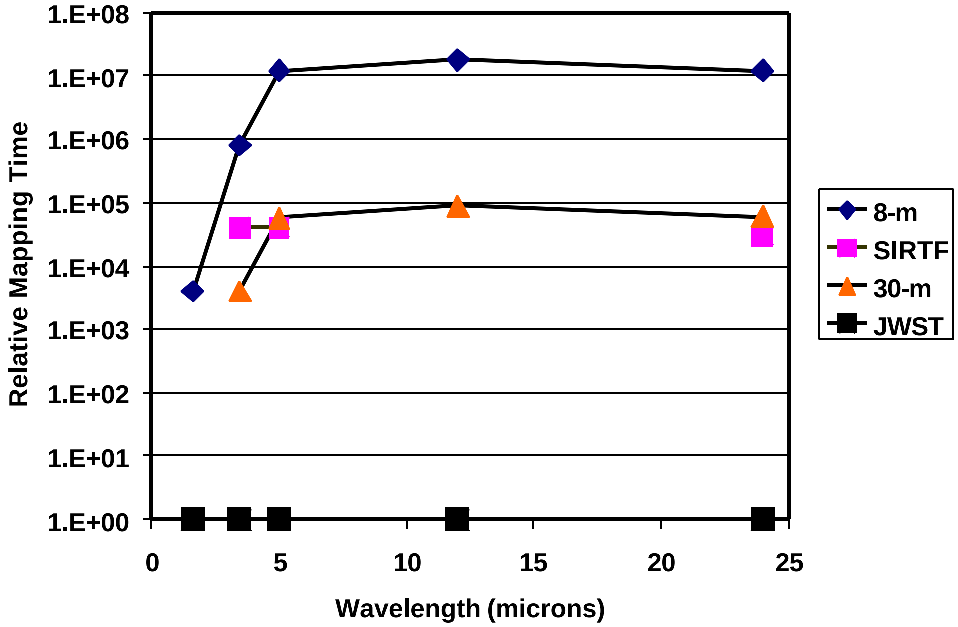


**Figure 1. Gain in survey speed (astronomical capability) of MIRI on JWST over Gemini, SIRTF, and a 30-m diffraction limited groundbased telescope. Survey speed is measured in number of pixels mapped on the sky. For the last telescope, the performance at the shorter wavelengths depends critically on Multi-Conjugate Adaptive Optics and hence has not been included in the figure.**

Both SAFIR and SUVO (and of course TPF) make major advances in our capabilities that do bring significant potential for paradigm shifts. The gains with SUVO have been discussed in terms of a “discovery efficiency.” For a spectrograph, this metric is proportional to the throughput times the wavelength coverage times the efficiency. For an imager it goes as the throughput times the field of view times the efficiency and is analogous to astronomical capability. As shown in Figure 2, even 4-meter aperture versions of SUVO have discovery efficiencies about two orders of magnitude improved over HST (Shull et al. 1999). SAFIR provides more than two orders of magnitude increase in sensitivity over SIRTF (in part because it resolves out the far infrared background and hence penetrates far beyond the SIRTF confusion limit) (e.g., Lester 2003). As shown in Figure 3, even with existing detector arrays the gains in astronomical capability will be three to six orders of magnitude.

Forecasting the future is never easy, and in planning for the future of NASA missions we normally place missions in priority order instead. I hope that this panel places a high priority on approaches that maximize the potential for significant shifts in our astronomical paradigms.

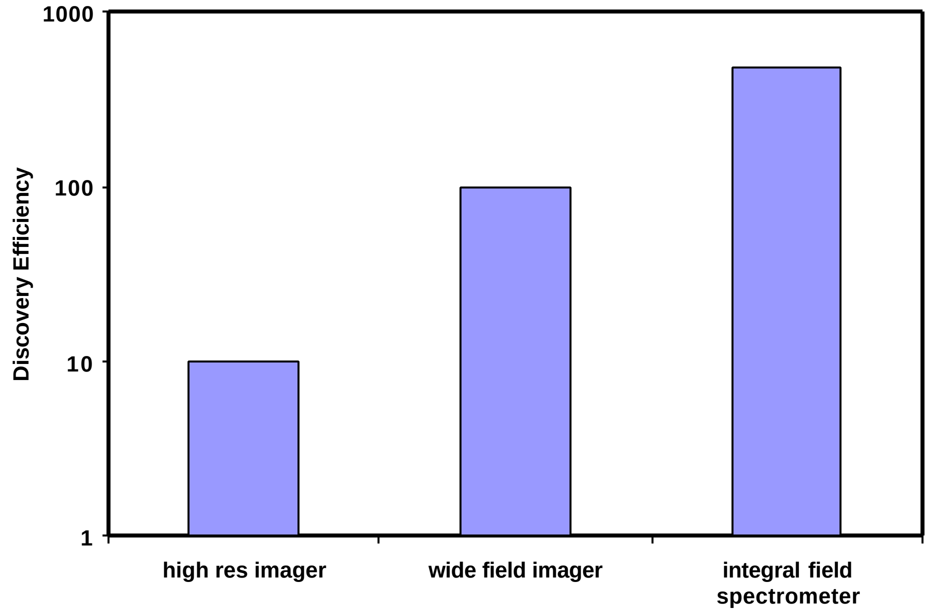


**Figure 2. Gain in discovery efficiency over HST for the three instruments proposed for a SUVO Ib mission. This mission category includes a versatile suite of instruments but is well within current technological and cost constraints. For imagers, discovery efficiency is analogous to astronomical capability. Data from Shull et al. (1999).**

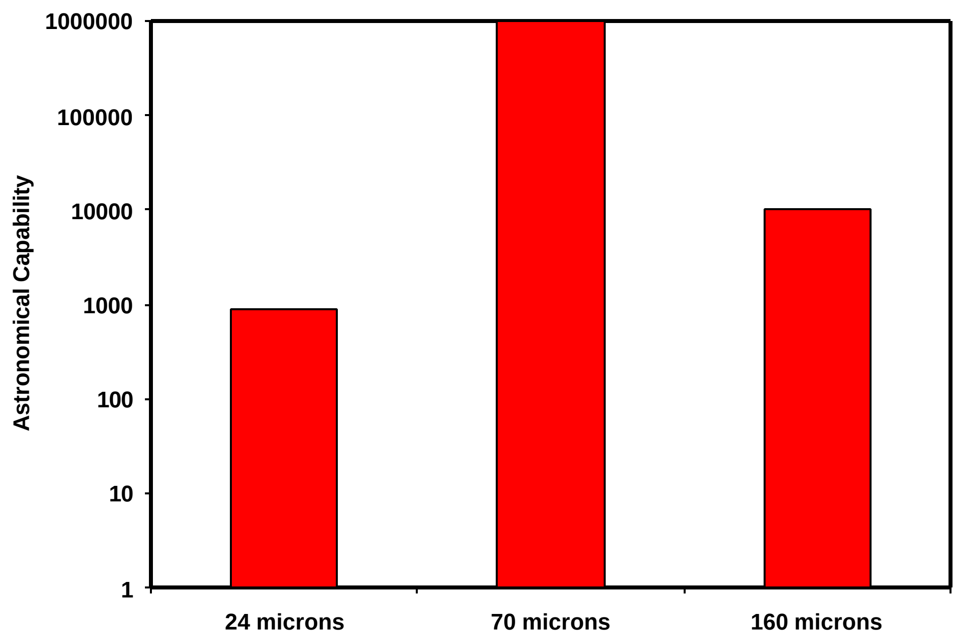


**Figure 3. Gain in astronomical capability of SAFIR imaging compared with JWST (24mm), SIRTF (70mm) and FIRST (160mm). No gains in detector format or throughput are assumed over the baseline missions. After Lester (2003).**

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